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Discussions

Interactive comment on "Surface currents and upwelling in Kerguelen Plateau regions" *by* M. Zhou et al.

Anonymous Referee #1

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The manuscript 'Surface currents and upwelling in Kerguelen Plateau regions' by Zhou et al. describes the circulation around the Kerguelen Plateau and its influence on dissolved iron. Sadly I can only find very little of scientific significance in this manuscript, with many methods being inappropriate and wrong. Recent work by other authors has been almost completely ignored.

Here are some of the major points:

1) The authors try to calculate eddy diffusivities from surface drifters released around the Kerguelen Plateau. This analysis has the following issues:

The Lagrangian integral time scale has been chosen as 4 days according to work by Inoue (1950) and Ogura (1952). There has been a lot of recent work showing that this choice makes no sense – to calculate realistic eddy diffusivities it is necessary to integrate over a much longer time to take into account effects of the mean flow (e.g. Klocker et al. (2012, 2012b), Griesel et al. (2010, 2014)).

To calculate believable eddy diffusivities it is necessary to use a large number of floats, especially in complex regions such as the Kerguelen Plateau (e.g. Klocker et al. (2012) or Griesel et al. (2014)). The only solution I can think of to this problem in this particular region is either to use numerically advected floats with velocity fields derived from satellite altimetry (e.g. Sallee et al. (2011)) or the use of a combination of ocean models and observations (e.g. LaCasce et al. (2014)).

Eddy diffusivities are always positive definite, and eddy diffusivities are not the same as the energy transfer between EKE and mean flow due to Reynolds stresses (which can have either sign)! The eddy diffusivity is what matters for the distribution of tracers (and hence dissolved iron). This section is physically just wrong!

The results shown in Fig. 7 are just noise for the reasons explained in above comments. And obviously the components of Kxx and Kyy look different to the components Kxy and Kyx if the color scales are chosen different! To make sense of the components of the eddy diffusivity tensor it is also important to use a coordinate system related to the direction along the mean flow (e.g. Griesel et al. (2014)).

2) The authors talk about mean flows and paths of a good handful of surface drifters, but never mention anything about errors arising from using such a small amount of drifters over such a short time. Are these results really representative of the mean circulation? And why do the authors not use any supplementary information, such as is easily accessible from satellite altimetry? Without any error estimates these results are quite meaningless.

3) The authors use Sverdrup balance to estimate a northward transport. On the one hand this approach is very hand-wavy due to the lack of appropriate boundary conditions, and on the other hand the transport by mesoscales is completely ignored in this

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11, C2759–C2762, 2014

discussion.

4) The authors estimate upwelling, but do not even mention in a single sentence that other processes, such as submesoscale processes, are likely to have a much larger impact on the tracer distribution, especially in the top few hundred meters and downstream of bathymetry (e.g. Rosso et al. (2014)). So how much do the results calculated in this paper actually matter relative to other processes?

5) In the conclusion section the authors guess around on what could be responsible for getting dissolved iron off the shelf but come to no conclusion on which processes matter - so where's a new result?

Looking at all these flaws (and there are quite a few more not mentioned here), and not finding any substantial scientific progress in this paper, I really think that this paper should be rejected.

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